Anonymous Referee #1
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The current version of the paper requires significant modifications and rewrites in order to be considered for the Cryosphere journal. I think that this paper is more suitable for Geoscientific Model Development journal and suggest to authors to consider that journal for this paper. Overall, it looks to me that authors promote usage of the dynamics snow and organic layer in the process-oriented models, which sounds more like advances in the model that authors employed in their work.

We like to thank the reviewer for taking the time to read and review this manuscript and for a fast comment. Advancements of the previous manuscript are highlighted in red in the revised version of the manuscript.

As is written in the abstract and the last paragraph of the introduction section, and also explained in detail in the first paragraph of the discussion section, this paper has a clear research question: What are effects of short-term variability of meteorological measures on soil temperature in permafrost regions? The research gap behind is explained in the second and third paragraphs of the introduction section.

In general one can ask this question for any other ecosystem state or function, and a whole EU FP7 Project (www.carbo-extreme.eu) was funded to find answers, mainly on the carbon cycle, cf. Reichstein et al. (2013). There are several different approaches possible to answer such question, e.g. one could design lab or field experiments (cf. CARBO-Extreme project) in which the climate forcing to the ecosystem is manipulated. That would be really interesting to do also for the specific question here about effects on permafrost soil temperature, see also new discussion text lines 363-364. However, in reality the experimental design is very complicated (because the averages need to be conserved), very expensive, would be required at several locations, and, when looking at subsoil temperature, required also long periods. Therefore, we try to give first answers from a theoretical point of view, mimicking reality with a process-oriented land surface scheme forced by artificially modified climate forcing data. In doing so, it is for example much easier to keep the long-term average equal to the control, and we can see results at a continental scale including a huge range of baseline environmental conditions (climate regimes and soil properties).

The model used to address this question has already been developed and published:
Porada, P.; Ekici, A. & Beer, C. (2016) Effects of bryophyte and lichen cover on permafrost soil temperature at large scale, The Cryosphere 10, 2291-2315.

Therefore, we think that The Cryosphere is the right journal for addressing this research question.

Authors state that climate variability mostly impacts snow depth and the upper soil organic layer (SOL). Authors call SOL as cover and thermal diffusivity of lichen and bryophytes. These different notations confuse the reader right from the beginning. First describe the lichen and bryophytes. Do not assume that readers know everything about them. Spend more lines on the description in the introduction and methods sections. What are their spatial coverage and thickness? Why are they so important? Note, that SOL thickness and the level of saturation will determine soil temperature for the certain location and time.

We fully agree with the reviewer, and it is also see by the model experiments, that the properties of this near-surface vegetation is of major importance for soil temperature. Mosses and lichens are growing on top of any soil organic layer and respond dynamically to climate and CO2. That was also the reason why, in a major **previous** effort, a dynamic vegetation model of lichens and bryophytes has been included into the JSBACH land surface scheme (Porada et al., 2016). When applying this new "moss model" in the context of the present research question, we see the importance of this near-surface vegetation layer.

Wording and definitions, however, should be clear from the beginning. Most of the new "moss model" functions are described in the last paragraphs of section 2.1 and the reader is of course also referred to the Porada et al. (2016) paper in The Cryosphere which is also open access. Still we agree on taking more space in the introduction to clarify what is meant by lichens and bryophytes. For this, we introduce a new fourth paragraph to the introduction section, lines 54-64.

In Abstract. Where is the 1K higher temperature come from? Is this temperature difference uniform for every geographic location? Statements like 'less-pronounced' in the abstract looks like a hand-waving to me. Please use exact numbers (statistics) when making any statements in the manuscript.

This is the main result of the REDVAR experiment as shown in Fig 7 and described in section 3.4. In the revised version, we advance the sentence to the following:
"As a result, soil temperature is 0.2 to 1 K higher when climate variability is reduced under conserved long-term mean meteorological measures, depending on the location."

The last sentence of the abstract is to recall the overall conclusion of the paper as it has been discussed in the discussion section. In the revised version of the manuscript this sentence now reads as "Therefore, our results show that projected future increases in permafrost temperature and active-layer thickness in response to climate change will be lower i) when taking into account future changes in short-term variability of meteorological measures, and ii) when representing dynamic snow and lichen and bryophyte functions in land surface models."

I suggest to review the corresponding literature and cite previous work appropriately in this study. For example, recent work summarizing the models inter-comparison on modeling of snow (Wang et al., 2016). Recent work stressing the importance of the organic layer and coupling of the soil biogeochemical processes in the land system models (Jafarov et al., 2016). Once again changes in the SOL heat diffusion properties directly correlate with the level of saturation of soil for a specific year (see O'Donnell et al., 2009 and many others).

We thank the reviewer for these suggestions and advanced citations in the introduction and discussion sections. However, we would also like to point out that the importance of snow and soil properties on heat conduction as well as their dependence on environmental conditions has been studied and reported extensively during the past century and that it is not our aim to give a balanced amount of citations on that fact. For the research question of this paper it is more important to remind the reader on these functions and we think it would be more useful to give citations to textbooks for readers that want to learn about it.

In conclusion, there are improvement that has to be done through the entire paper. If resubmitted for the Cryosphere journal then the advancement in science has to be better stressed.

We hope that we could demonstrate the advancement in science that merits a publication in The Cryosphere by addressing a specific important research question.

References

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2.Jafarov, E. and Schaefer, K.: The importance of a surface organic layer in simulating permafrost thermal and carbon dynamics, The Cryosphere, 10, 465-475, doi:10.5194/tc-10-465-2016, 2016

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